

## MONITORING DISTANT FALLOUT: THE ROLE OF THE ATOMIC ENERGY COMMISSION HEALTH AND SAFETY LABORATORY DURING THE PACIFIC TESTS, WITH SPECIAL ATTENTION TO THE EVENTS FOLLOWING BRAVO

Merril Eisenbud\*

**Abstract**—The fallout from test BRAVO in March 1954 has had scientific, political, and social implications that have continued for more than 40 years. The test resulted in serious injury to the people of the Marshall Islands and 23 men on a nearby Japanese fishing boat. Prior to BRAVO there was insufficient appreciation of the dangers of fallout to people living downwind from surface or near-surface explosions of megaton weapons. In the absence of sufficient preplanning for fallout monitoring beyond the test-sites of earlier smaller yield tests, and as a result of the concern of the photographic film manufacturers, the Atomic Energy Commission Health and Safety Laboratory, now the Department of Energy Environmental Measurements Laboratory, was requested to develop a program of fallout surveillance. Beginning with Operation IVY in 1952, these surveys included aerial monitoring of the islands of the mid and western Pacific, as well as establishment of fallout monitoring stations in the United States and abroad. The first evidence of the post-BRAVO fallout was detected by a Atomic Energy Commission Health and Safety Laboratory instrument installed on the atoll of Rongerik, where 28 military personnel were stationed. The results of radiation surveys conducted immediately after BRAVO, as well as the reports of medical investigations, radioecological studies, and dose reconstruction that have been conducted by many laboratories over the years have been available from the beginning in unclassified form. However, from the time of the fallout, and continuing to the present, there have been many unanswered questions about what happened during the hours immediately after the fallout was reported. No formal investigation of the circumstances of the fallout was ever conducted, and there were serious misrepresentations of the facts in the official statements made at the time.

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### INTRODUCTION

#### Origin of the Health and Safety Laboratory

When the Atomic Energy Commission was formed in 1947, operating responsibilities for procuring needed

materials such as beryllium, uranium, and thorium was assigned to the New York Operations Office (NYOO). The plants that had produced these products during the war years had many industrial hygiene, health physics, and industrial safety problems that required attention, but the facilities were relatively small, compared to the those at Hanford, Oak Ridge, and Los Alamos, and could not be expected to develop the in-house expertise with which to deal with their health problems. The Commission therefore established within NYOO a Medical Division directed by Bernard Wolf, with a mandate to provide the contractors with the consulting and laboratory services that would be required. I was hired to develop the special laboratory and field capabilities that would be required. When Wolf retired after two years, I was named Director. The name of the unit was changed to the Health and Safety Laboratory with the acronym HASL (Eisenbud 1994a). Thirty years later the name was changed again, to the Environmental Measurements Laboratory, by which it is known to this day, almost fifty years after its formation.

The laboratory was unique for the time, in that it contained all the specialties required to administer a health and safety program including physicians, industrial hygienists, radiological safety specialists, instrument designers, and sanitary and safety engineers. When it was established, and for many years afterwards, it was the only laboratory operated directly by the AEC and not under contract with industry or universities. This was done because the Commission wanted the laboratory to be fully responsive to its immediate programmatic needs.

HASL would not have been involved in fallout monitoring but for an unexpected development shortly after the testing program began in January 1951. No national system of radiation monitoring had been established despite knowledge that after the first test explosion at Alamogordo in July 1945, fallout many hundreds of miles away caused damage to film produced by Eastman Kodak Company (EK). Based on that experience (Webb 1949), the company had installed radiation monitors in the air supplies for its production facilities. An increase in radioactivity was detected at the Rochester plant thirty-six hours after a test air burst in Nevada.

The AEC Division of Military Applications requested that HASL investigate the report, and that was

\* 340 Carolina Meadows Villa, Chapel Hill, NC 27514.

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the beginning of HASL's involvement in the fallout studies of our nation's nuclear weapons testing program (Eisenbud 1990). It was Friday, it had been snowing throughout the area and temperatures were well below freezing. We decided to obtain snow samples to help us determine the extent and the amount of fallout. We knew many people to whom we could turn to help collect snow in quart sized ice-cream containers for hand-delivery to our laboratory in Manhattan. The HASL staff worked around the clock to collect the equipment needed to evaporate and beta-count the water samples. (There would be better ways of doing it today!). In this way we succeeded in demonstrating that surprising amounts of fallout could occur over large areas thousands of miles from a relatively small air burst. The incident attracted attention, not only within the Commission, but among the officials of EK who put AEC on notice that they would hold it responsible for any damage caused by the tests unless they were provided with information that would make it possible for the industry to protect its processes. HASL was requested by headquarters to do whatever was necessary to learn about long-range fallout. What I didn't know until much later was that there were intelligence organizations within the government that were well equipped to monitor fallout and were, in fact, doing it for other reasons. One advantage in our being assigned to monitor overtly was that our work could be unclassified. In all the time we were monitoring fallout from the tests we had essentially no contact with the "other monitors."

### OPERATION JANGLE

By the spring of 1951, preparations were underway for tests of two surface and underground devices in Nevada (Operation JANGLE). Although relatively small, the tests were expected to result in much higher levels of fallout than the previous ones, which were exploded high in the air or on towers. I was briefed on the plans for the tests and learned that monitoring would be conducted only within 320 km (200 miles) from the explosion. I was of the opinion that intensive monitoring should extend to 800 km (500 miles), and HASL was accordingly assigned responsibility for the 200–500 mile (320–800 km) annulus around the test-site. HASL also decided to develop a worldwide monitoring program that could at least provide semi-quantitative information about where and when fallout would occur. This led to deployment of the gummed films that made it possible for the many participating stations to mail a daily collection sheet to HASL. It is now many years since the gummed film collectors were used but the data we collected continue to be useful in dose reconstruction efforts that have become necessary in recent years (Beck et al. 1990). The monitoring plans for JANGLE included mobile monitoring teams, staffed by military personnel assigned to HASL for that purpose. The teams were equipped with aircraft that transported the personnel to stations picked because they lay across the expected path of the fallout. These

teams measured airborne dust and gamma radiation, and particulate deposition. The results of our monitoring activities in the 320–800 mile annulus confirmed our concern about possible high levels of fallout. Salt Lake City, about 640 km away, received exceptionally high levels of fallout from the small-yield Nevada tests, with the dose through 1955 estimated at 1.60 mGy (Eisenbud and Harley 1956).

My late colleague John Harley and I assembled the data we were collecting into articles that were published periodically in *Science* (e.g., Eisenbud and Harley 1953, 1956). At that time there was little public interest in the levels of fallout to which the public was being exposed, and our reports received little attention in the media. The media seemed more interested in the drama of the Nevada tests themselves. Although the data we were collecting could be published in the open literature, there were many things about the tests that remained classified. In advance of the tests this included the time of detonation, much to the frustration of the media photographers. However, the tests took place at about weekly intervals, approximately at daybreak. Many of the reporters assigned to the test program would still be at the Las Vegas gambling tables when members of the test organization would come through the lobby in field clothes. That a shot was imminent could no longer be kept a secret and the reporters would leave for the various vantage points atop hills from which they could see and photograph the explosions. Classification rules were often puzzling to me.

The results of the Jangle fallout studies resulted in HASL being drawn further into plans for Operation IVY to be conducted in the Marshall Islands by Joint Task Force 132 (JTF 132), beginning on 1 November 1952. The HASL group continued to believe that tests with yields equivalent to millions of tons of TNT had the potential to produce dangerous levels of fallout at great distances. It was a simple matter to scale the results of the fractional kiloton Jangle tests to tests in the range of ten megatons, but there was surprisingly little concern about the subject. Some even said the blast would project the debris into outer space from which it would not return. I never understood the process of psychological denial that led the weapons group to be so cavalier about the potential danger from the many petabecquerels of fission products and induced radionuclides that would be released by the test explosions. There was, however, at least one noteworthy exception within the military organizations: LCL N.M. Lulegian, an Air Force meteorologist. We had had several conversations about the possibility of lethal long-range fallout, and in November 1953 he wrote a meteorological analysis in which he confirmed that lethal levels of fallout could occur over an area of 5,000 square miles ( $1,953 \text{ km}^2$ ) a few hours after a 10 megaton thermonuclear explosion (Lulegian 1953). A copy of his classified report, which essentially confirmed what we believed, was sent to me at the time but a few days later was mysteriously ordered to be returned

to the originating office. The report was not declassified for more than forty years.

## OPERATION IVY

By the summer of 1952, our gummed film network had been extended worldwide, thanks to the cooperation of the Air Weather Service and other government overseas organizations that cooperated by manning the stations. The U.S. Weather Bureau was particularly helpful in making these arrangements. However, we found little interest on the part of the Task Force in monitoring beyond the atoll of Eniwetok where Operation IVY was to be conducted. However, the officers on the staff of the Commander-in-Chief-Pacific (CINCPAC) learned of our concerns through John Bugher, who had recently joined the staff of the AEC Division of Biology and Medicine. The Marshall Islands were then a United Nations Trust Territory assigned to the United States for administration, and CINCPAC was responsible for the security of the people of the Marshall Islands. CINCPAC took the position that all atolls in the Marshall Islands and other island groups should be monitored after each test. At HASL, we too believed that the atolls should be surveyed, but how could it be done? By the early summer of 1952 we decided that it would be feasible to monitor each island by low level (62 to 156 m) overflights, using a HASL designed and built portable scintillation detector (Cassidy 1954; Cassidy et al. 1957). The instrument had a rapid response time and a logarithmic range from  $2.5 \times 10^{-9}$  to  $2.5 \times 10^{-4}$  C kg<sup>-1</sup> (0.01 to 1,000 mR h<sup>-1</sup>). After several conferences it was decided that CINCPAC would relieve JTF132 of the burden of supporting our mission, which would require a fleet of long-range aircraft. Calibration of our monitoring system took place first at the Nevada Test Site and later at Eniwetok where there were many "hot spots" at the sites of previous tests (U.S. AEC 1953). The system designed for IVY was the first major use of aerial surveillance methods.

In addition to the aerial monitoring instruments, we also designed and built one dozen continuously recording gamma radiation monitors to be placed on key atolls. These also had a logarithmic response and a range of  $2.5 \times 10^{-9}$  to  $2.5 \times 10^{-5}$  C kg<sup>-1</sup> (0.01 to 100 mR h<sup>-1</sup>). We reasoned that the land based monitors would help us to vector the aerial sweeps. By early October we were fully prepared and John Harley, Al Breslin, Mel Cassidy and myself, all from HASL, proceeded to the Marshall Islands. On the way, I stopped for another conference at Pearl Harbor and was requested by the CINCPAC staff to make every effort to survey all the Pacific island groups. I promised to do our best but emphasized the importance of giving the highest priority to the islands downwind of the tests. We then moved to our base on Kwajalein where we would be housed close to the aircraft assigned to us. I made my headquarters on the U.S.S. *Estes*, a Command and Control vessel that was the Task-Force flagship. During the last days of October, the HASL staff continued to test our aerial survey instruments by flying over

the bomb craters of Eniwetok atoll. We confirmed that we had stable and reproducible calibration factors to relate the aerial measurements to measurements made on the ground. An interesting feature of the "scintillilogs," which was what we called the aerial survey instruments, was that they employed two-channel tape recorders, one for a frequency modulated radiation detection system and the other for voice-recording of our position.

At daybreak on 1 November 1952, MIKE, the first multi-megaton thermonuclear explosion, took place. Our plan called for me to leave the *Estes* by helicopter within 1 h post-detonation, proceed to a task-force carrier, and leave immediately from there for a 2-h flight to Kwajalein. Mel Cassidy and myself were scheduled to begin the surveys at first-light the following morning using two PBM's, sturdy long-range flying boats. We were each to sweep about 1,000 miles in the downwind direction, descending to about 60 m for surveillance of the low coral islands. When we departed just before dawn the radiation background on Kwajalein was about 10 times normal, indicating that a part of the cloud had passed our way.

During my first flight I made measurements above fifteen exquisite atolls with Polynesian names like Taongi, Utirik, Wotho, and Bikini, but there was no way to avoid frequent squalls that interfered with our visibility and made the flight uncomfortable. We repeatedly passed through minor parts of the bomb-test cloud and particles of radioactive dust would impact on the leading edges of the aircraft, which caused an increase in the radiation levels within the cabin. We soon found that most of the particles would wash away when we flew through a rain-squall. I recorded only minor elevations of the radiation levels on the atolls despite the fact that I had chosen the sector that I thought would have the heaviest fallout. After six days of searching for the MIKE fallout we concluded that it had probably deposited in the vast spaces between the atolls. CINCPAC was delighted with the negative results. However, there would be other multimegaton explosions in the Pacific and the good fortune might not always prevail. Because the islands were so small and so widely separated, we needed new ways to study the intensity of fallout from such tests.

During the months after IVY there was much speculation about the whereabouts of the MIKE debris. There was very much less world-wide fallout than expected, and we had found very little fallout in the immediate downwind area of Eniwetok. Could the debris have been blown into outer space? It was far more likely that the MIKE dust particles had been injected into the stratosphere. If so, at what rate would the dust enter the troposphere where it would mix rapidly and be readily detectable before it deposited on the surface of the earth? At that time we did not know the rates of exchange between stratospheric and tropospheric air.

One way to obtain information would be to sample the stratospheric air at an altitude of at least 30,000 m. I favored sampling by electrostatic precipitation, a method that had long interested me. Our group had no experience

with high altitude balloons, but the Weather Bureau came to our assistance, as they frequently had done in the past. Although it was already October 1953, and the first test of the CASTLE series was scheduled for 1 March 1954, we did succeed in designing and building an electrostatic precipitator system and completing a series of about twelve flights into the stratosphere at the desired altitude (Holland 1959). We did identify  $^{90}\text{Sr}$  in the dust samples we recovered but the samples were too few to permit an inventory of the stratospheric radioactive dust burden. However, that venture did lead HASL to later conduct a more elaborate stratospheric sampling program that was highly successful for many years (Feely 1960).

## OPERATION CASTLE

Late in the Fall of 1953 I returned to Pearl Harbor to discuss our participation in CASTLE, which would involve tests of several multimegaton yield thermonuclear devices, beginning on 1 March 1954. This time the tests would take place on the atoll of Bikini, where Operation CROSSROADS, the first post-war series of tests took place in 1946. I was warmly received by the CINCPAC staff, who I knew were both pleased and reassured by our work during IVY. We had developed a symbiotic relationship with CINCPAC even though our interests and objectives were very different. CINCPAC had the obligation to assure that the inhabitants of the Pacific Islands were safeguarded. The HASL objective was to develop a better quantitative understanding of the fallout phenomenon, the severity of which we believed was being greatly underestimated by the AEC and the Department Defense.

The HASL plan for CASTLE was to use the same general methods developed for IVY except that the ground instrumentation would be improved and would be located on islands on which military weather stations would be located. As before, a HASL representative would be stationed aboard the *Estes* to obtain the data needed to plan the aerial sweeps. John Bugher, after conversations with us, wrote to General Clarkson, the Commander of the joint task force (JTF7) assembled to conduct the tests, recommending that an evacuation capability be provided for the atolls nearest the tests, but he replied that this would not be necessary because they would not fire any devices unless the safety of the islands was assured. This proved to be a serious mistake.

At HASL we recognized that it was probable that the fallout would again deposit on the vast expanses of ocean water between the atolls. We conceived the idea of laying slicks of oil that could catch and retain fallout long enough to permit measurement from the air. We even went so far as to use the Brookhaven reactor to irradiate dust that could be dropped on the oil slicks in tests undertaken by Al Breslin off the New Jersey coast. The winter weather did not cooperate with us and we started to transfer the tests to Pearl Harbor. This proved not to be necessary because on 1 March 1954, shot BRAVO was fired from a position at the northern end of Bikini atoll

and within a few hours there was no longer any doubt about the fact that fallout from megaton weapons can produce lethal or near-lethal levels of exposure at great distances from an explosion.

About 6 h after the burst the Air Weather Service personnel on Rongerik, an atoll about 160 km east of Bikini, noticed that the recording gamma monitor had pegged at its limit of  $2.5 \times 10^{-3} \text{ C kg}^{-1}$  (100 mrem  $\text{h}^{-1}$ ). The information was immediately sent to Al Breslin the HASL representative aboard the *Estes* who then passed the information to me at the New York laboratory. The procedure we adopted, which had been approved by both JTF 7 and CINCPAC called for immediate aerial confirmation by aerial surveillance using the aircraft assigned to our team. When Breslin attempted to send the necessary instructions to Kwajalein where the aircraft were based he was denied use of the radio facilities aboard the *Estes*. This resulted in a blackout of information for a critical period of time. After 24 h, during which I received no additional information, I called John Bugher at AEC headquarters and learned that he had no knowledge of what had transpired. I then flew to Washington to confer with him, and by the time I arrived he had learned that there had been some sort of fallout in the forward area but had no details. On the *Estes*, confirmation of the high radiation levels on Rongerik was obtained the following day independently of Breslin. The 28 Air Weather Service personnel were evacuated by air about 24 h after the fallout occurred. They had received an external whole body dose estimated to be 0.78 Gy.

More alarming was the fact that the surveillance aircraft detected much higher radiation levels on the populated atoll of Rongelap about 210 km west of Bikini. An evacuation by boat was ordered and 258 people were removed from the atoll about 50 h after the fallout occurred. It was later estimated that 67 people on Rongelap had received whole body doses of about 1.75 Sv. The thyroid doses among children less than 10 y old averaged about 20 Sv (Lessard 1984). The thyroid dose estimates must be regarded as uncertain because they were based on radiochemical analysis of urine samples that were not collected until at least 2 wk after exposure (Conard 1992). Those were days before gamma spectrometry was available for radioiodine analysis and little has been published about the chemical separations procedures used at the time. There could have been no measurements of the short-lived radioiodines, since these would have already decayed.

There were several more tests in the CASTLE series, but these were uneventful. HASL completed the assignment given it by CINCPAC and was able to document the fallout on 40 atolls and Islands (Breslin and Cassidy 1955). It was a surprise to many of those associated with a 1994 Congressional hearing on BRAVO that all the information developed by HASL had been published in an unclassified report only a few months after the tests were concluded (Committee on Natural Resources 1994).

## THE JAPANESE FISHING BOAT

Unknown at the time of the Bravo explosion was the fact that a Japanese fishing boat, the *Fukyu Maru* (Lucky Dragon) was on its way to its home port of Yaizu in Japan after being subjected to fallout about 180 km East of Bikini. Unobserved by the precautionary sweeps conducted by the Task Force before the test explosion, 23 men aboard the 100-ton vessel were fishing for tuna when they saw the BRAVO flash. Fallout began to deposit on the ship about 4 h later. The men knew they were in proximity to the restricted zone announced by the Task Force but there is no evidence that they were within it. After encountering the fallout they decided to return home, which they reached on 16 March. By that time the men were showing signs of radiation sickness, but they had no understanding of what was happening. During the trip home they maintained radio silence which they later explained was because the crew had been poaching in Indonesian waters the year before and had been apprehended and jailed. They said they were afraid that if their whereabouts became known they would once again be arrested and detained. By the time they reached Yaizu all were suffering from severe symptoms of acute radiation sickness. The tuna they caught were found to be heavily contaminated and were disposed of by land burial. Thus began the first post World War II crisis between the U.S. and Japan, known locally known as the Great Tuna Panic.

At the Atomic Energy Commission the staff learned about the Lucky Dragon in the same way as the rest of the world—from the news broadcasts. John Morton, a thoracic surgeon from the University of Rochester was then Director of the Atom Bomb Casualty Commission in Hiroshima (now the Radiation Effects Research Foundation). He was instructed to assess the situation and shortly afterwards placed a conference call to John Bugher and myself in which he asked for help in dealing with the radiological aspects of the problem. I left for Tokyo immediately, totally unprepared for what I found on arrival after 40 h (air travel was at that time quite a bit slower than today). The Japanese nation was very angry about the incident. They were the only people to have been hurt by American atom bombs, first during the war and now again. Many of the Japanese scientists were making sensational statements to the media. No one in Japan understood the technical implications of the event, and it didn't take long for me to realize that I had a difficult educational mission ahead of me. Morton was in an especially difficult position. He knew nothing about radiation medicine but was invited to Japan because he had developed a reputation as an effective scientific administrator. He was in no position to answer the kinds of questions that were being asked. How should the doses to the fishermen be calculated? What radioactive substances were in the fallout? What was the allowable level of radioactivity in tuna fish? How is radiation sickness treated?

The incident was the first major interruption in the otherwise smooth relationships that had existed between

the U.S. and Japan since the end of World War II. The problem was made worse by statements made by some Americans that the fishermen were spying, despite good evidence to the contrary. The fishermen were very sick and getting sicker by the day. Yet some Washington officials made statements that minimized the effects. Some U.S. officials wanted U.S. physicians to treat the fishermen, but the Japanese physicians refused assistance because they were offended by the suggestion that they could not provide the men with the best of medical care. It was certainly true that the Japanese had unique experience with the acute radiation syndrome among the people of Hiroshima and Nagasaki. In this post-war period the Japanese were offended by the suggestion that they could not assume responsibility for the care of their citizens.

The dispute between the Japanese physicians and John Morton persisted for several days. The public clamor increased. The Japanese Foreign Office established a committee of physicians and scientists to deal with the scientific aspects of the problem and requested that Morton and myself deal only with them. This proved to be a very wise move. I soon found that, despite the problems that existed between the Japanese and American physicians, I was quickly developing rapport with the physicists and chemists, most of whom were highly qualified but had not had any association with U.S. science since before the war.

I soon was able to define the three major problems with which we were confronted:

1. The clinical management of the 23 fishermen. In my opinion there was very little help to be offered. The U.S. had no methods of treating radiation sickness that were not already known to the Japanese;
2. Determining the dose received by the fishermen. This included the dose delivered by external radiation during the fourteen days on the contaminated ship and the dose from absorbed internal emitters. The Japanese scientists indicated that they were willing to allow us to collaborate in estimating the internal dosimetry; and
3. The concerns of the Japanese that the Pacific tuna were becoming dangerously contaminated.

By the time of my arrival, the Japanese physicists had already estimated the external radiation dose received by the fishermen using pre-war electrometers that nevertheless gave quite reliable results in their expert hands. However, they did not know how to calculate the dose from the internal emitters. In 1954, even the composition of bomb fallout was still secret, but by the time of my arrival considerable progress had been made by Japanese radiochemists in determining the principal radionuclides present in the particles recovered from the fishing boat. Much of this was done by Kenjiro Kimura, a distinguished chemist who had attracted attention after the nuclear bombings of Japan by concluding correctly that the Nagasaki bomb was built with plutonium because he found traces of that element in soil samples obtained

from an area where rainout had occurred. In my first meeting with him he told me that his radiochemical analysis of the "Bikini ashes" led him to conclude, also correctly, that BRAVO involved fast neutrons acting on  $^{238}\text{U}$ , a fact that was classified at the time.

I had requested that urine samples be collected from the fishermen and these were sent to HASL for analysis. Much to the surprise of all of us it was found that only minimal amounts of  $^{90}\text{Sr}$  was deposited in their bodies even though they had spent 14 d living in such a highly contaminated environment. Several months later one of the fishermen died of serum hepatitis and Kimura was able to confirm our estimates of only minimal exposure to internal emitters (Kimura 1956). The serum hepatitis was believed to have resulted from infection in the course of many blood transfusions. Thus, although the death did not result directly from the effects of radiation, it certainly should be considered an indirect effect.

The Lucky Dragon incident had implications beyond concern about the health of the fishermen. When reports of the incident were received in the U.S., The Food and Drug Administration decided to monitor all incoming tuna from Japan. This was a reasonable decision under the circumstances and would have created no problems because there was no general contamination of the Pacific tuna. However, in response to this the tuna fish companies gave notice that they would not pay for shipments of fish from the Japanese tuna fleet unless the fish were certified as non-radioactive before they left Japan. There was no mechanism for providing certification at the time. Consumption of tuna in both Japan and the U.S. dropped precipitously as a result of the extensive publicity. The Japanese tuna fleet of 1,000 vessels, with an annual catch worth 26 million dollars, suffered grievously as a result.

### SOME UNANSWERED QUESTIONS

The events surrounding the BRAVO fallout remain obscure in many respects. This is particularly true with respect to many details at the time of the test, some of which I have mentioned in this article. A more lengthy account appears elsewhere in more detail (Eisenbud 1990). I am unaware of any official investigations of the facts surrounding the fallout. Did an unexpected wind shift occur, as has been commonly said, or was the meteorological "window" that existed too narrow for safety? Why wasn't an evacuation capability provided for the atolls most likely to be in the path of the fallout? AEC had in fact requested the task force to provide such a capability. Why were Breslin's communications with Kwajalein and New York interrupted?

Evacuations of the military personnel and the residents of Rongelap were not announced until ten days after they took place and were stated to be "according to plan as a precautionary measure" (U.S. AEC 1954a). Since no mention was made of the severity of the fallout, this was a clear understatement of the facts. No further announcements were made until March 24 when it was

announced that the restricted area around the testing area was being expanded to provide greater assurance against unauthorized entry into the restricted zone (U.S. AEC 1954b). The big shock to Morton and myself came on 1 April, when we learned that President Eisenhower and Admiral Strauss, AEC Chairman, had held a televised news conference on the subject. It was said that the fishermen "must have been well within the danger area," although there was no evidence that this was so (U.S. AEC 1954c). By the time of the press conference the men were suffering from severe beta burns of the skin, in addition to the fact that their blood counts were still dropping and their lives were in danger. At the 31 March (1 April in Japan) press conference and in the accompanying public announcement it was said the "skin lesions are believed to be due to the chemical activity of the converted material in the coral rather than radioactivity. . . ." In other words, the burns were attributed to the action of the calcium oxide produced by the intense heating of coral in the fireball!

This last statement had a devastating effect on the relationships Morton and I had developed with the Japanese scientists and physicians. It was only natural that they should assume we were the sources of these statements. To the credit of Ambassador Allison who was capably representing the U.S. during this difficult period, he was angered by what was said in Washington.

Morton and I had ceased to be useful in Japan and we left for Eniwetak to report on what we had learned and to become informed about developments in the Marshall Islands. Although I was bitter about the course of recent events, I could take comfort in the fact that the fallout was detected by an instrument that HASL believed was necessary for the safety of the Marshallese people. If the instrument had not been installed, the fallout might not have been detected for several days and the radiation injuries to the servicemen and the people of Rongelap would have been far more severe, perhaps fatal. And, had the evacuation capability recommended by HASL been provided by the Task Force, the doses received would have been greatly reduced. Moreover, HASL had surveyed and recorded the radiation levels on more than 100 atolls and islands after the IVY and CASTLE tests.

One of the remaining mysteries of the BRAVO affair was that no official inquiry was conducted. It was not until 40 y later that I was requested to testify before a congressional committee at the request of representatives of the Republic of the Marshall Islands. When I reported that to my knowledge there had been no formal inquiry of the circumstances of the BRAVO fallout, the committee Chairman, Representative George Miller stated "If the Navy runs a tugboat aground, we have a board of inquiry!" (Committee on Natural Resources 1994). That is in fact a poignant statement. Forty-two years later, without the benefit of a timely inquiry, when so many of the participants have passed from the scene, and with memories becoming increasingly fallible, a detailed explanation of what happened on the

morning of 1 March 1954 is likely to remain a gap in history.

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